Title: A tectonic-rules based mantle reference frame since 1 billion years ago – implications for supercontinent cycles and plate-mantle system evolution.
Journal: Solid Earth
Authors: R. D. Müller et al.
Reviewer: Rhodri Davies (ANU).

In this manuscript, authors: (i) present a new global plate tectonic reconstruction from 1 Ga to the present day, in the mantle reference frame, that was developed using tectonic rules-based optimisation strategy; (ii) use this as a constraint for mantle flow models, to understand the evolution of mantle structure over this time, thus connecting tectonic motions at Earth's surface to the underlying mantle. These topics will obviously be of interest to the readership of Solid Earth.

There are several important results in the paper, three of which seem particularly far-reaching:

1. *Supercontinent cycles:* results support an orthoversion evolution from Rodinia to Pangea, with Pangea offset approximately 90° eastwards relative to Rodinia. This is very important result: it is the opposite sense of motion compared to previous studies based solely upon paleomagnetic data and is, obviously, more consistent with the "tectonic-rules" incorporated through the optimisation.
2. *The evolution of mantle structure in response to these extended plate motion reconstructions:* model predictions point towards 5 main stages for deep mantle structure over the past 1 Byr: (i) a broad network of hot basal ridges between 1000 and 600 Ma; (ii) the formation of a short-lived degree-2 basal mantle structure with upwellings centred on the poles from 600-500 Ma; (iii) a transitional phase during which the north polar basal structure migrates southwards and gradually morphs into an extensive Pacific centred basal structure, while the south polar structure is dissected by subducting slabs and disintegrates into a network of ridges between 500-400 Ma; (iv) a Pacific-centred degree 1 structure that is stable from 400 – 200 Ma; and (v) a basal degree 2 structure, post 160 Ma, with upwellings centred beneath the Pacific and African domains. This succession of mantle states is distinct from previously proposed models, as would be expected given the differences in the plate motion histories. It implies a mobile deep mantle and has important implications for our understanding of Earth's thermo-chemical evolution and how this links to the surface.
3. *Reference frames:* The NNR rotation reference frame, enforced by most existing *mantle circulation models*, is reasonably ok, which adds confidence to the results of these previous studies. This is a relief!

Given the above, I feel that the manuscript is clearly worthy of publication in Solid Earth. However, there are a few changes that I would recommend, which are outlined below, that I feel would further strengthen the manuscript. My main comments are presented first, followed by more minor suggestions.

Thank you for the opportunity to review this work. I hope the authors find my comments useful.

Best wishes,

Rhodri

Main comments:

1. As noted above, one of the main results from the optimised plate tectonic reconstructions is the orthoversion evolution from Rodinia to Pangea, albeit in a different direction to earlier work by Mitchell *et al.* (2012). This is potentially highly significant and, if correct, it is of fundamental importance for our understanding of coupled plate-mantle models. Given its importance therefore, I would urge the authors to make more of an effort to find independent observations that support their findings. Is this result supported by other observations? If such observations are currently lacking, or the authors are unaware of any, at the very least they should present testable hypotheses from their models that would allow others to later test the validity of the different results. What is it about the coupled plate-mantle models that differ for example, between the OPT end member and the PMAG end member? Would the models, for example, predict a different dynamic topography evolution? Would your models predict a different magmatic record in the continents (where a record of such magmatism could be preserved)? What else is different? I think it's vital to draw out these differences so that others can build on your work.
2. Authors spend a lot of time comparing their model predictions to present-day mantle structure, as imaged through seismic tomography. I will outline my concerns with the specifics of approach that has been used below. However, in the context of this paper, such comparisons are almost irrelevant: I found them to be a distraction from the paper's main message. Previous studies have shown that deep mantle structure is only sensitive to the past 200-250 Myr of plate motion histories – this has been pretty much clear since the work of McNamara & Zhong (Nature, 2005) and perhaps even Bunge et al. (PTRS, 2002). Given that the different reconstructions examined here are all similar at 200 Myr, there really is not much to be gained (at least in my opinion!) by examining present-day structure. The focus of results and discussion should be more on the differences predicted between each scenario as a function of time, as suggested in point 1 above. The authors almost acknowledge this themselves when stating – "it is noteworthy that the unoptimized model PMAG, not representing a mantle reference frame, reaches an equivalent accuracy to the optimised models OPT1 and OPT2. This reflects that the present-day mantle structure is largely the results of the post-250 Ma subduction history." I'd suggest removing these comparisons or transferring across to the supplement.
3. Tectonic rules: I realise these have been outlined by Tetley et al. (2019), but it would be good to see a little more of a summary in the current paper, to provide valuable background material. In the context of the current paper, it would also be good to comment on whether such assumptions/rules are reasonable, back to 1 Gyr.

Minor points:

1. Abstract – the approach of Bower et al. (2015) amounts to more than a surface boundary condition (thermal structure, dip angle etc… are also imposed). I would therefore recommend changing line 24 to reflect this. Perhaps "use it as a constraint on mantle flow models", or similar?
2. Line 65 – … a combination of relative plate motion and *constraints provided by* mantle convection models.
3. Line 80-85 it is stated that the assumption of fixed LLSVPs is based on an apparent correlation between the reconstructed eruption sites of LIPs and kimberlites, from the work of Torsvik, Burke and others. However, using powerful statistical approaches, a number of studies (e.g. Austermann et al. GJI, 2014; Davies et al. EPSL, 2015), have shown that this correlation is not robust, whilst a follow up study by Doubrovine et al. (2016) essentially shows the same (i.e. you cannot conclusively state that plumes are forming at edges over LLSVP interiors). I find it surprising that these studies, which support the authors conclusions for mobile deep mantle structure, are not cited or discussed. Alongside the models presented, these studies provide a solid basis for challenging the fixed LLSVP hypothesis of Torsvik and others.
4. Line 91 – probably fair to cite work by Bull et al. (EPSL, 2009) and Davies et al. (EPSL, 2012) here too.
5. I found the jump in logic from Line 101 to Line 102 hard to follow at first. Do "alternative modes of supercontinent formation" really belong in a subsection on LLSVPs? Having read it a few times, I see

the link, but perhaps a separate section, or a sentence explicitly connecting these two aspects, would be helpful.

6. Line 232 – model setup – you limit the age of the lithosphere to 80 Myr when constructing the thermal structure of plate, but still use a half-space model. Why is this? Why not use a plate model, where thickness changes are small beyond this age anyway? Are your results sensitive to this age? If so, it's probably worth explicitly acknowledging that this is the case.
7. Line 236 – just a flag that the CMB temperature used in these models is very much towards the lower end of current estimates. The Di is also higher than I'd have expected. I'd recommend that authors provide a justification for their choices.
8. Line 247 – 256 – there seems to be spurious use of bold font in places.
9. Viscosity: I find it difficult to convert that beautiful (!) equation describing your viscosity into an understanding of the range of values in the model and their depth and lateral sensitivity. Could you add a plot showing the depth average and range of values? This will help a reader to place your results in the context of other studies with different rheological approximations.
10. Comparisons between model predictions and imaged structure: as noted in the main points above, I do not feel that these comparisons add anything to this paper and find them a little distracting. Dropping these comparisons would free up space to discuss your exciting results in more depth. As well as this, I have a major concern with how such comparisons are undertaken. Seismic velocity is non-linearly dependent on temperature, composition and phase. Furthermore, tomographic models have limited and uneven resolution. None of these important factors are considered in the comparisons that you present. Several previous studies (e.g., Bull et al. EPSL 2009; Schuberth et al. G3, 2009; Davies et al. EPSL, 2012) demonstrate that they need to be considered when comparing models with tomography.
11. Just a comment. I REALLY liked Figures 2 and 3. They were very useful for a geodynamicist that is not an expert in plate motion reconstructions. It was valuable to be able to directly compare the different reference frames.
12. Lines 394 – 410. This is a very interesting insight. I don't know the answer of the top of my head and haven't had time to appraise the literature, but are these trends supported by models that examine the evolution of trench retreat under various scenarios (I'm thinking of work by Goes, Garel, Van Hunen, Capitanio, Moresi, Holt, for example)?
13. Line 407 – who doesn't get excited by the "zippy tricentenary"? It's not a term I'll forget in a hurry!
14. Figure 4 – remove duplicate scale bars (unless I'm missing something)?
15. Line 443 – you mention that there are some periods of relatively large RMS speeds and attribute these to potential artefacts in the reconstruction. Could you say a little more here to help a non-expert? What type of artefacts are these? And why are you confident that they are not present at other times?
16. Line 470 – you use the term ridges and nodes. What is meant by nodes here? Are they simply ridge intersections? If so, perhaps use ridge intersections instead, or define nodes on first use (given that you also use nodes in a different context elsewhere in manuscript).
17. Line 490: … basal mantle structure *with upwellings* centered on the north and south pole…
18. Fig. 11 – I could not easily make out the bright red dots. Perhaps enlarge? Or add crosses or similar?
19. Line 643 – it is explicitly mentioned here, but it is also mentioned elsewhere in the paper: short subduction zones have the capacity to roll back faster than long subduction zones. In general, this is true, but I think the reality is a little more nuanced. If the downgoing plates are young, trench retreat is limited, even for short subduction zones. In other words, the magnitude of trench retreat does not only depend on the length of a subduction zone, but also its age (as well as complications arising from overriding plates etc…). This is explicitly covered in a pre-print here https://www.essoar.org/doi/10.1002/essoar.10508606.1. Potentially something that's worth looking at further down the line in your extended reconstructions is whether you see evidence for these dependencies in your reconstructions.
20. Line 696 – strcturee – structure.

21. Line 710-712 – it's probably fair to cite work by Davies et al. (EPSL, 2012) and Bower et al. (G3, 2013) here.
22. Lines 714-720 – with the comparisons of slab depths it's important to acknowledge that your models do not include phase transitions, which are important in dictating the form of slab transition-zone interaction.
23. Line 789 – I think a little too much credit is given to the study of Davaille and Romanowicz (2020) here. I'm not denying it's a wonderful study, but it builds on concepts and inferences from many previous studies that are not cited. I would recommend perhaps giving some credit to some earlier work in this area, alongside the work of Davaille and Romanowicz.

Anyway, that's it from me! Hopefully these points are useful and will allow the authors to further improve what I felt was already an excellent study.